but dangerous to the touch. It grows from 2 or 3 inches to 10 or 15 feet in height, and emits a disagreeable odour. Says a traveller: 'Sometimes, while shooting turkeys in the scrub, I have entirely forgotten the stinging tree till I was warned of its close proximity by its smell, and have often found myself in a full forest of them. I was only once stung, and that very lightly. Its effects are curious. It leaves no mark, but the pain is maddening, and for months afterwards the part when touched is tender in rainy weather, or when it gets wet in washing, &c. I have seen a man who treats ordinary pain lightly rolling on the ground in agony after being stung, and I have known a horse so completely mad after getting into a grove of the trees that he rushed open-mouthed at everyone who approached him, and had to be shot. Dogs when stung will rush about whining piteously, biting pieces from the affected parts.'" Mr. N. E. Brown, of the Royal Gardens, Kew, made an interesting contribution (*Gard. Chron.*, *loc. cit.*, p. 567) of his personal experience concerning the virulent effect of the sting of this species in the palm-house at Kew. TOKUTARO ITO.

Tokio, January 25.

## The Sailing-Flight of Birds.

SINCE Mr. F. W. Headley urges (February 16, p. 511) readers of NATURE to make observations on the flight of the albatross, possibly a few remarks may be of interest from one who, as a student of aërodynamical problems, has carefully watched such wonderful performances.

One point which has always struck me is that the albatross almost invariably flies in immense circles, ever varying in size and direction. Sometimes the bird will be high overhead, then, swooping down on a curve, will skim closely over the tops of the waves, then suddenly rising again will float away to perhaps half a mile off, gradually sweeping around, and perhaps again attaining a considerable elevation. It seems quite impossible to decide, from observation, whether the elevation is gained from uptending winds. Without doubt, the bird takes every advantage of each puff or eddy he can find, but he does *not* progress, as in Mr. Mallock's figure, steadily from wave to wave, rising and falling with the waves. Nor does he, so far as one can judge, invariably rise when facing the wind, and *vice versa*. That albatrosses, as well as other birds, seem always to soar in circles, may be due to the circumstances that, for instance, in following a ship, they have to circle round and round in order to keep near it, not being able to fly at such a slow speed as that of the ship (and therein is a hint to our cross-Channel aviators). So too vultures and eagles may soar around, not wishing to depart from the district which they are watching. But the question to which I have long wished for a reply is, "Can birds soar in a straight line?" I remember many years ago seeing, on the Nile, flocks of pelicans gliding along on outstretched wings. Now these birds were progressing up the river, performing actual journeys, but I cannot remember whether they were actually soaring in a straight line all the time.

I have frequently noted, what is patent to all, that in calms there is no true soaring; also I feel sure that there is no kind of motion of the wing such as Mr. Hearn suggests. One thing is certain, and that is that a soaring bird, especially the albatross, always progresses at great speed. Having once obtained the initial impulse, there is so very little head resistance offered to forward motion by the bird that the speed slackens but little. He progresses, as described by Langley, in the manner of a skater skimming over thin ice, travelling so fast that the ice has not time to break.

Much of interest on this matter is to be found in the old annual reports of the Aëronautical Society. In that for the year 1868 there is an interesting discussion in which Mr. Young is quoted as saying that he had noticed "the hollow form of birds' wings; these were not planes; indeed, a little consideration would show that the curved surface is better than a true plane. . . . He concluded that the best means of flight is by a curved wing." A great deal has been said on this subject during the last few years, but how many have read these old, and often instructive, discussions? B. BADEN-POWELL.

## The Non-simultaneity and the generally Eastward Progression of Sudden Magnetic Storms.

### *Fact.*

IT will be necessary, first, to my regret, to direct attention to some further errors in Dr. Krogness's communication in NATURE, December 8, 1910, p. 170, to which I made reply in the issue of January 5, p. 306. He questioned the correctness of our time of beginning of the disturbance, May 8, 1902, as recorded on the horizontal intensity curve at Potsdam, viz. 12h. 0m. Greenwich mean civil time, whereas his determination for the same station was 11h. 58m. In my reply, I stated (*idem*, p. 307) that Dr. Krogness must have made an error somewhere, for, upon repetition of our time scalings, based upon the data supplied us by the Potsdam Observatory, we got our identical result. I next wrote to the Potsdam Magnetic Observatory and requested that the time be scaled with all possible accuracy from the *original* magnetogram. Under date Potsdam, January 20, 1911, Dr. Venske gives the time in question as 11h. 59·7m., hence within 0·3m. of our time, but differing 1·7m. from that of Dr. Krogness.

Furthermore, in compliance with a circular request, I am receiving almost daily from observatories over the whole globe fresh data on the fifteen sudden disturbances, July 29, 1906, to September 25, 1909, first investigated by Mr. R. L. Faris for the five Coast and Geodetic Survey observatories. It will be recalled that Mr. Faris's data had furnished important testimony on the question as to the strict simultaneity of abruptly-beginning disturbances. Both Drs. Chree and Krogness have attempted to break down this testimony, but the data received thus far from other observatories are bearing out the conclusions previously drawn.

Table I. contains the time data for the very same disturbances chosen by Dr. Krogness. Confining our attention to the horizontal intensity disturbance curves, just as he does, we have first the mean time of beginning as derived from the five Coast and Geodetic Survey observatories (Faris's data, the means being formed by Dr. Krogness; for the last disturbance the record at Sitka was missing, so that in the mean only four observatories are embraced). Next is given the times of beginning for the Potsdam Observatory, first as derived by Dr. Krogness and employed in his communication (*idem*, p. 171), next as recently scaled by Dr. Venske at Potsdam, using the *original* magnetograms. Forming the differences P (Potsdam), U (United States), it is seen that for the Krogness scalings there are three plus differences, two minus ones, and one zero, resulting in an algebraic mean of but +0·03m. The case is, however, different for the next column, which depends upon the Venske, *i.e.* the *original* data of the Potsdam Observatory; there are now five plus differences and but one minus, the algebraic mean being +0·47m. or 0·44m. higher than that of Dr. Krogness. These differences (P-U) are small quantities, to be sure, but the interesting point is that, in every instance, for the Venske figures they are in the same direction as determined by me from the five Coast and Geodetic Survey observatories alone, and as published in *Terrestrial*

TABLE I.—*Greenwich Mean Civil Times of beginning of Disturbances in the United States and at Potsdam.*

| No. | Date | United States | Potsdam | | P–U | | Direction |
|---|---|---|---|---|---|---|---|
| | | | Krogness | Venske | K | V | |
| | | h. m. | m. | m. | m. | m. | |
| 20 | 1906, July 29 | 19 56·12 | 57 | 56·5 | +0·88 | +0·38 | E. |
| 24 | 1907, July 10 | 14 22·92 | 22·5 | 23·2 | −0 42 | +0·28 | E. |
| 25 | Oct. 13 | 7 42·36 | 42·5 | 43·7 | +0·14 | +1·34 | E. |
| 28 | 1908, Sept. 11 | 7 20·82 | 20·3 | 20·7 | −0·52 | −0·12 | W. |
| 30 | Sept. 28 | 8 42·00 | 42 | 42·6 | 0·00 | +0·60 | E. |
| 31 | Sept. 29 | 1 31·68 | 31·8 | 32·0 | +0·12 | +0·32 | E. |
| | Algebraic mean ... ... | | | | +0·03 | +0·47 | |
| | Numerical ,, ... ... | | | | ±0·35 | ±0·51 | |

*Magnetism*, vol. xv., p. 231, Table VIII., Nos. 20, 24, 25, 28, 30, 31. The letters E (motion of disturbance eastward or plus motion) and W (minus or westward motion) given in the last column are as taken from Table VIII. spoken of; note how the plus sign is linked each time with E and the minus with the W. Surely the most captious critic will hardly contend that this is mere chance.

As judged by the Venske data, Dr. Krogness's time scalings are in error from −0·5m. to +1·2m., and, on the average, +0·43m., his general tendency being to measure the Potsdam time *too low* by almost 0·5m. It must hence not be surprising that he failed to detect the generally eastward progression in the times between the United States and Potsdam, and was, instead, led to negative results; the difference of half a minute is precisely on the order of the required quantity.

Dr. Krogness, in his communication (*idem*, p. 171), unwittingly revealed also that the time scalings of his chief—Prof. Birkeland—were likewise untrustworthy. In order to get some definite information regarding the methods employed, Prof. Birkeland was next appealed to directly, but unfortunately in a reply received from him he failed to answer my question. It is recommended that all time data which appear in Prof. Birkeland's vol. i. (Norwegian Aurora Polaris Expedition, 1902–3) be used with extreme care by anyone who wishes to look into the matter of simultaneity of abruptly-beginning disturbances. I am not surprised now that Prof. Birkeland was unable to reach any definite conclusion himself on this interesting and important question, for his data lacked the necessary refinement.

Since I am on record as believing that no implicit reliance is to be placed upon simply one observatory, no matter how excellent its instrumental equipment and methods may be, Table II. is next given for the fifteen disturbances spoken of above; in this all the observatories are embraced the data of which have been received to date, February 10, excepting one the time scalings of which differ occasionally 10m. or more from near-by institutions, and are doubtless subject to some error. The "North American group" embraces the six observatories Honolulu, Sitka, Baldwin, Agincourt (Canada), Cheltenham, and Porto Rico, the mean geographic position being 36·3° N., 101·7° W., of Greenwich; the "European group" gives the mean times for the seven observatories Stonyhurst, Greenwich, Uccle, Wilhelmshaven, Munich, Potsdam, and Katharinenburg, the mean geographic position being 52·4° N., 13·6° E., of Greenwich. It will be noticed that Kew is not included, for the simple reason that, although Dr. Chree scaled the required data some months ago, he has not yet published them nor forwarded them to me. The Greenwich data were received the earliest of all, viz. January 23, and those of the distant Observatory of Katharinenburg on February 9. The numbers attached to the various entries in Table II. correspond to those in my Table VIII. (*Terr. Mag.*, vol. xv., p. 231).

TABLE II.—*Greenwich Mean Civil Times of beginning of Disturbances in North America and Europe.*

| No. | Date | North America | | Europe | E–N | Direction | |
|---|---|---|---|---|---|---|---|
| | | h. | m. | m. | m. | U.S. and Europe | U.S. alone |
| 20 | 1906, July 29 | 19 | 56·15 | 56·52 | +0·37 | E. | E. |
| 21 | Aug. 7 | 13 | 38·50 | 38·78 | +0·28 | E. | E. |
| 22 | Dec. 21 | 21 | 30·70 | 30·87 | +0·17 | E. | E. |
| 23 | 1907, Feb. 9 | 14 | 12·72 | 12·79 | +0·07 | E. | E. |
| 24 | July 10 | 14 | 22·85 | 22·79 | −0·06 | W. | E. |
| 25 | Oct. 13 | 7 | 42·50 | 44·17 | +1·67 | E. | E. |
| 26 | 1908, Mar. 26 | 17 | 41·25 | 41·65? | +0·40? | E.? | W. |
| 27 | Aug. 19 | 0 | 14·35 | 14·31 | −0·04 | W. | W. |
| 28 | Sept. 11 | 7 | 20·87 | 20·47 | −0·40 | W. | W. |
| 29 | ,, 11 | 21 | 47·48 | 46·51 | −0·97 | W. | E. |
| 30 | ,, 28 | 8 | 41·97 | 42·20 | +0·23 | E. | E. |
| 31 | ,, 29 | 1 | 31·20 | 32·56 | +1·36 | E. | E. |
| 32 | 1909, May 14 | 4 | 55·40 | 56·77 | +1·37 | E. | E. |
| 33 | Sept. 25 | 8 | 39·70 | 38·43 | −1·27 | W. | W. |
| 34 | ,, 25 | 11 | 41·80 | 40·33 | −1·47 | W. | W. |
| | Mean of positive values ... ... | | | | +0·66 | 9 E.<br>6 W. | 10 E.<br>5 W. |
| | ,, negative ,, ... ... | | | | −0·70 | | |
| | Mean regardless of sign ... ... | | | | ±0·68 | For a great circle arc of 75° | |
| | ,, ,, ,, ... ... | | | | ±0·82 | For a great circle arc of 90° | |
| | ,, ,, ,, ... ... | | | | ±3·3 | For a complete circuit | |

The last two columns ascribe the direction of progression of the disturbance according to the sign of the difference E–N, plus meaning east. First the direction is given as derived from the present investigation, which depends upon data over the region from Honolulu, 158° W., to Katharinenburg, 60·6° E., and next as obtained previously from the five Coast and Geodetic Survey observatories alone (Honolulu, 158° W., to Porto Rico, 65·4° W.). Comparing the two columns, it is seen that only in three cases out of fifteen, viz. Nos. 24, 26, and 29, do the letters clash; in other words, in 80 per cent. of the cases the directions, as determined from the limited portion covered by the United States observatories, agree with those now gotten for a considerably larger region. Moreover, Nos. 24 and 29 exhibit the interesting fact that while the disturbance each time progressed eastwardly in Europe just as it did in the United States, yet the mean time of occurrence for each is less in Europe than in North America. These are precisely similar cases to the disturbance of May 8, 1902, which seemingly began in the Atlantic and Europe, and then travelled eastward, being felt last in the United States. When the data for the observatories in Asia are available, the actual direction of progression of the two disturbances Nos. 24 and 29 will be known better. This shows, as I have already pointed out, how important it is to know approximately the region where the disturbance originated (cf. *Terr. Mag.*, vol. xv., p. 20). The result from No. 26 is more or less doubtful, evidently the point of beginning being not sufficiently sharp at all stations; three of the observatories mark their times doubtful, and two omit giving them.

Both columns unite in showing that the eastwardly progressing disturbances predominated over the westwardly ones in the ratio of about two to one. The average difference E–N, regardless of sign, is 0·68m., which was the average time required for a disturbance to pass from the mean position of the North American group to that of the European, or over a great circle distance of 75°. If the disturbance continued to progress at this rate, and were to make a complete circuit of the earth, it would take 3·3m., hence on the order of the quantities already announced. The linear velocity here concerned would be about 200 km. per second.

The available data on the non-simultaneity and progression of abruptly beginning disturbances have now been subjected to so many severe tests that it is difficult to see how anyone with an open mind can any longer doubt that some important discoveries concerning magnetic disturbances have been made. I should, indeed, be glad to be informed of any other facts in terrestrial magnetism which have stood as well the tests applied.

Dr. Chree, unfortunately, in his paper before the British Association last summer, and again before the Physical Society of London on November 11 last (Proceedings, vol. xxiii., part i., December 15, 1910 [49]), devotes chief attention to pointing out difficulties in explanation. He seems more concerned in determining why, according to his ideas, the phenomenon should not be rather in finding out whether it *is*. One of the chief purposes of my paper was to arouse further investigation on the part of others. Dr. Chree could not have done better than immediately to have published his own data in the same open manner that Mr. Faris had done. Instead, he labours to discredit the Coast and Geodetic Survey observations, and withholds his own from public scrutiny. In half the interval of time between the first and second presentation of his paper, had Dr. Chree chosen, he could have had at his command data from Europe and Asia which, combined with his own, would have served admirably to have tested the main contentions. He might thus have been credited with a really helpful contribution to the subject; but no such attempt has been made.

In view of the discussions which have arisen with regard to time data from present magnetograms, I have made request of each observatory for a statement of the method employed. From the reports thus far received, it is found that no institution has made a more earnest attempt to allow for all sources of error than is the case at the Coast and Geodetic Survey observatories. It is evident that in several instances equal care, for one reason or another, is not given by others, but, judging from the fresh interest aroused by the present investigations, there is every reason to expect considerable improvement hereafter. This may be a sufficiently useful end to have achieved, even if nothing else had resulted from the researches.

### *Theory.*

The hypothesis of ionic currents which I have employed in the study of magnetic disturbances thus far treated is based on the existence of a primary set of electric currents circulating around the earth overhead. Quoting from my paper No. 1 (*Terr. Mag.*, vol. xv., pp. 122–3):—

"Since magnetic observations made at various points on the Earth's surface have revealed the existence of a definite system of atmospheric electric currents, it follows at once that if the atmosphere is made more conducting at any point, an extra current will be started and set in motion by the pre-existent electromotive force or its equivalent. The direction followed by the new current depends upon its origin, upon the direction of the electromotive force at that point, and upon the deflecting effect of the Earth's magnetic field and of the Earth's rotation on the electric carriers. In other words, while we shall look chiefly to extra-terrestrial agencies for ionising the air and thus splitting it up into carriers of positive and of negative charges, we look to the atmospheric electric field and to the Earth's rotation for furnishing the energy necessary to drive the ions over the Earth and by their motion produce the effects observed during a magnetic storm."

In No. 3 (*idem*, vol. xvi., p. 34) I summarise the evidence available regarding the outside electric field as based upon the harmonic analyses of the earth's magnetic condition by Adams, Schmidt, and Fritsche. I show that its general characteristics are very similar to the supposedly internal magnetisation of the earth. The outside currents, if negative ones, would have to circulate around the earth from west to east, hence in the same direction as the rotation of the earth. Starting with these currents, I find it possible to account for the earth's own magnetisation if the earth's average magnetic permeability is on the order of 135 as referred to air and for a magnetising force of about 0·0024 C.G.S. This value, while seemingly large, is not impossible, judging from the experiments of Lord Rayleigh and of C. Baur on iron, using small magnetising forces. Furthermore, it must be borne in mind that we are absolutely ignorant as to what effect the great pressures existing at but a few kilometres below the surface may have on the permeability of magnetisable substances—possibly the effect of increased temperature with depth may be completely annulled by the comparatively more rapid increase in pressure.

In brief, I have set up the hypothesis that the earth is chiefly an electromagnet, the magnetising currents being outside, and consisting of negative electric currents circulating overhead in the same general direction as that of the earth's rotation. I follow out the consequences, and show that this hypothesis harmonises with the Gaussian analysis, from which it had been hitherto almost universally concluded that the magnetising causes must be contained chiefly *inside* the earth.

By thus putting the magnetic state of our planet primarily in the control of *outside* electric currents, many of the outstanding problems of terrestrial magnetism are greatly simplified. Any variation, periodic or spasmodic, in the intensity and direction of the magnetising currents must, of course, be followed almost immediately by corresponding changes in the earth's magnetisation. It thus becomes clearer now how, in the space of but a few minutes, such great changes can occur in the magnetic condition of our planet as are experienced during magnetic storms. Anything that will cause a change of conductivity in the strata containing the outside currents will evoke changes in the latter, which in turn are revealed in effects on our magnetic needles. There may be many such causes—the theory need not restrict itself to any one, as, for example, kathode rays.

Analysing the type of disturbance of which the one of May 8, 1902, may be taken as typical, it is seen that we have before us but a miniature reproduction of the earth's own magnetisation. In brief, a system has been found which will produce magnetic disturbance effects precisely similar to the permanent magnetic effects referred to the earth (*idem*, vol. xv., pp. 25–30, 117, and vol. xvi., pp. 33–48).

The type of disturbances which Prof. Birkeland refers to "equatorial" currents is thus found to be merely a general disturbance of the entire magnetic condition of the earth, of such a simple character that the first harmonic may give a sufficiently complete representation of the observed perturbations. The theory advanced in my papers is that the same electric-current system which may have to be held accountable for the production of the earth's magnetisation will also suffice for the production of the disturbances considered. I propose the name, therefore, of "simple magnetic perturbation," in place of Prof. Birkeland's "equatorial magnetic perturbation," adding the words "positive" or "negative," just as he does, according to whether the general effect is to increase momentarily the earth's magnetisation or to decrease it. Prof. Birkeland and I are not in agreement as to the direction in which the outside currents must go to produce the observed magnetic effects (*idem*, vol. xvi., pp. 33–48).

The careful reader will not fail to observe that the theory, as above briefly outlined, is considerably different from that which Dr. Chree imputed to me on p. 51 of his paper cited above. Furthermore, the calculation which he says he is unable to follow, although others have done so, was merely a preliminary attempt to account for the observed progression of sudden disturbances and to get some idea as to the order of the altitude at which the supposed currents would have to circulate. If Dr. Chree has something better to offer I shall be glad to know it. Such interest is being manifested now on all sides, that it will doubtless not be long before a fairly satisfactory theory will be forthcoming. Someone must make the attempt, however, to rear a structure; not all of us are willing to rest contented with merely pulling down. One correspondent has hit upon another promising clue, which is at present being tested. In the meanwhile, I believe our hypotheses have amply justified themselves by the many new questions raised and the fresh incentives given to investigation. L. A. BAUER.

Washington, D.C., February 10.

*Postscript.*—From NATURE of February 2, just received, it is noticed on p. 461 that Prof. Schuster presented a paper before the Royal Society on January 26 entitled "The Origin of Magnetic Storms," in which a critical examination is made of the theory that magnetic storms are caused by streams of electrified corpuscles ejected from the sun. Prof. Schuster, after various calculations, reaches the following conclusions:—

"If magnetic disturbances are produced by rays emanating from the sun, it can therefore only be in an indirect manner. We may imagine that the injection of corpuscles ionises the upper portions of the earth's atmosphere, and consequently renders the already existing electromotive forces more effective, or we may imagine that the approach towards the earth's magnetic field of highly conducting material containing ions of both kinds acts by induction. The effect of such induction would primarily be an increase in the horizontal and a diminution of the vertical forces, while the currents induced in the earth, tending to diminish the horizontal forces, would, owing to the inertia, die out more slowly, so that a semi-permanent effect would be left after the storm."

It will be seen that the theory as set forth in my paper above is entirely in harmony with Prof. Schuster's conclusions; it is, in fact, largely based on his previous researches. He and I are in agreement that the real origin of our magnetic disturbances is to be referred primarily to an outside electric system situated somewhere in our own atmospheric regions. I go one step further, and place in the same region the chief origin of the earth's own magnetisation.

In conclusion, it will be well to point out that the method used in my Table II. above to get the average rate of progression of sudden disturbances does not accentuate the actual time differences between distant stations, but tends rather to diminish them, as was the case with storms Nos. 24 and 29. In brief, as I have already hinted, the precise method of grouping of stations cannot be a fixed one, but must vary with the region in which the disturbance originated. When the data from the remaining parts of the globe have been received, this matter will become more evident.
L. A. B.

Washington, D.C., February 13.

### Colliery Warnings.

May I say a word about colliery warnings to point out that it is not the high barometer that is of any importance, but the dryness of the air? It happens in our country that the high barometer and dry air generally come together.

Gas explosions in coal mines are trivial, and they occur on an average of more than one per day. It is when there is sufficient dust to make the explosion spread over a big area that an explosion is serious, almost without exception. Dry air is the danger, and should be the basis for "colliery warnings."
John Harger.

University Club, Liverpool, February 21.

In all collieries more than 600 or 700 feet in depth the air is always dry—somewhat drier in cold than in warm weather—and, consequently, coal-dust is always present in the workings provided there is no natural "seepage" of water into them. In these circumstances, the one essential element of a great explosion is always present.

Blasting shot, when fired under certain conditions, and comparatively small volumes of explosive gas when ignited, will always raise and ignite coal-dust the quality of which, as regards its contents in volatile matter and ash, lies between certain upper and lower limits. On the other hand, shots are always being fired, and larger or smaller accumulations of explosive gas are always being formed here and there in mines of this class, quite irrespective of weather conditions.

All that can be said as regards the influence of weather is that, other things being equal, a coal-dust explosion is more likely to occur in cold weather, when the mines are driest, than in warm weather, when they are not so dry, and with a falling rather than with a steady or rising barometer.

These subjects were fully discussed at the very inception of the coal-dust question, and Mr. Harger might do well to study what was then said about them.

As the issuer of "Colliery Warnings" so frequently advocates that special attention be paid to the condition of mines when the barometer is rising rather than when it is falling, I may perhaps be allowed, in this place, to correct a statement which lately appeared in a letter to Nature, written by "The Author of the Warnings," to the effect that Mr. R. H. Scott and I were amazed (*sic*) to find that fire-damp was frequently reported to have been found in mines even when the barometer was steady and rising. We were not amazed, for we knew by the actual experience of one of us that, in consequence of falls of roof, damage to trap-doors, stoppings, brattices, and so on, which are amongst the commonest incidents in mining, the ventilation often becomes so stagnant at certain critical points that the air becomes explosive at or near these points before the defects can be rectified.

As a matter of fact, the principal province of the firemen is to guard against this very contingency. Consequently, when we saw appearances of fire-damp reported time after time with a steady or rising barometer, we experienced no surprise, but, perhaps rather unfortunately, considering the use the "Author of the Warnings" has made of the sentence in which we merely recorded a fact without further comment, we did not stop to explain what passed in our own minds at the time, but proceeded in the next following sentence to show that, when the records of a large number of mines were compared, the effect of these casual irregularities was practically eliminated altogether. It would have been fairer if the "Author of the Warnings" had quoted one more sentence (Quarterly Journal of the Meteorological Society, October, 1874).
W. Galloway.

### The Hydrogen Spectrum.

The colour of the light which is emitted from the capillary of a vacuum tube containing pure hydrogen is the familiar vivid, deep pink, the prevailing tint being due to the predominating brilliance of the red line ($H\alpha$). When the same capillary is viewed from one end, however, the colour is a very pale pink, indeed, nearly white.

This interesting effect appears to be due to the different relative intensities of the lines $H\alpha$ and $H\beta$ in the two cases, for while $H\beta$ (and probably each of the other lines in the primary series) has an intensity appropriate to the length of the column of gas in the capillary, $H\alpha$ appears to suffer reduction.

In the course of some observations on the secondary spectrum of hydrogen, during which I had occasion to use the tube when placed against the slit of the spectroscope in the usual way, and also end on, I was greatly interested in this apparent variation in the intensities of the lines mentioned. The explanation which suggests itself is that $H\alpha$ is weakened by absorption in traversing the column of gas (although this distance is only about 5 or 6 cm. in my tube), but it is difficult to understand why this absorption should be specially selective for $H\alpha$ and not equally effective in the case of $H\beta$, which is also a very brilliant line, and to which one would imagine the same argument would apply. There appears to be no reason for attributing the effect to polarisation.

I have seen no notice of this effect, but I imagine it must be quite familiar to spectroscopists, and perhaps one of them who has devoted special attention to the hydrogen spectrum may be able to throw some light on the matter.
Charles W. Raffety.

Beechcroft, 2 Park Hill Road, East Croydon,
Surrey, February 22.

### Life and Habit.

On p. 505 of Nature for February 16, in a review of a new edition of one of Samuel Butler's books, these words appear:—"therefore the apparently unpractised but perfect pecking of a newly-hatched chick proves that the chick has done it before," &c.

Now, I have tried many experiments with chicks hatched out singly and away from a hen, but never has any chick attempted to peck until shown how to or made to walk over food which tickled its toes, and my opinion is that a chick might die of starvation while surrounded by food unless taught what to do. They are quick to take a hint, and will imitate the motion and action at once if a bent finger be worked up and down like the head of a bird when pecking; and, if once they feel a bit of food within their beaks, they know what to do with it as well as a new-born babe knows how to suck when anything is placed in its mouth, though they have never done it before.
W. H. M.

February 20.

Your correspondent's remark is interesting, but, of course—as he himself indicates in connection with the babe—it does not invalidate Butler's argument. It can hardly be doubted that observation and imitation do not cover the ground, and that there is something calling for explanation in what is called "instinct"—a word which, it must be admitted, only disguises our ignorance of what it is. Butler's theory that "heredity is memory" is at least worth consideration.
The Reviewer.